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**ADDRESS**

DELIVERED AT THE OPENING MEETING OF THE



**Debating Society of Army Medical Officers, Aldershot,**

*APRIL 2nd, 1883,*

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BRIGADE-SURGEON MUSCHAMP AND GENTLEMEN,

In appearing before you on this occasion I have to express my high appreciation of the honour you have done me in requesting I should inaugurate the opening of this Society of Army Medical Officers, for the purpose of reading papers on professional topics, and promoting discussion thereon, with special reference to military medical practice. This Society, if carried on with adequate spirit, cannot fail to be of much advantage, not only in increasing the information, and giving order and precision to the opinions of those who rightly use it, but also in improving their management of the medical affairs of the army, both hygienic and curative.

The ideal of a debating society is that the members should have acquired a sufficient amount of information on the subjects which it is formed to discuss, to enable them to appreciate the facts and reasoning of the author of a paper read before them, to test the validity of his conclusions, and, when the topic has already secured attention, to compare the author's views with those held by previous writers and observers upon it, so that their differences being submitted to an intelligent criticism, matters which are inoperative in bringing about the result, or merely accessory to it, may secure that weight only to which they are entitled, and the issue between the opposing views be narrowed, as far as the information at the time will admit. There are, as you are aware, not a few questions in medicine on which the evidence is still so incomplete as to lead to considerable variety of interpretation by men of different turns of mind ; but there are also others, some of them of much importance, the accumulated evidence upon which, if rightly interpreted and

applied, should before now have led to a greater unanimity regarding them.

Medical officers, especially those who have been but a short time in the service, may become unsettled by the circumstances in which they find themselves, which are, in many instances, so different from what they were accustomed to previously; they may be at a distance from other medical officers, with limited duties, but few books, and deprived of many of those instruments and appliances which now enter so much into the ordinary detail of medical and surgical practice, and of the accessory branches of science; while the frequent moves they may be subject to discourage the formation of any collection of instruments for carrying on special investigations. No doubt all these are deterrent of continued application, but a man of active mind will soon rise superior to them all, and, if of an inventive turn, will often be able to improvise from the materials about him instruments and appliances to serve the ends for which they are required sufficiently well, however unpromising at first they may seem to be. There is no faculty more important for a medical officer on service to cultivate than this, of making the best use of whatever may be at hand, when, from circumstances beyond his control, those appliances to which he was accustomed are not available.

It may be asked, how can a man under the circumstances just mentioned contribute to the advance of medical science? Much that can directly advance medical knowledge, it is true, cannot be expected from him; but the very leisure he enjoys affords an opportunity of thoroughly investigating the cases under his care, and of comparing them with the descriptions of authors who have written on the subject, whose works may be within his reach. An exercise of this kind, while it shows a pretty close agreement between the actual cases and the authors' descriptions, will often, also, bring out differences more or less marked, which it is important for the young practitioner to become aware of, and, what is of still greater consequence to him in his future career, he will be educating himself as an observer, and, on being moved to another position affording more extended opportunities, will be able to enter upon them with greater chances of success than he could otherwise have done. As with disease itself, so with its causes and accessories, the variety of circumstances under which an army medical officer has to meet these, including climate, season, exposure, food, and exercise, and the various morbid influences connected with them, present numerous points for investigation, which, if carefully examined, will prove both interesting and valuable.

In the examination of individual cases of disease in the present day, the thermometer, sphygmograph, ophthalmoscope, and other instruments, have rather thrown into the background the aids to

diagnosis afforded by the countenance, attitude, and motions of patients, together with their general condition, which was much trusted by able practitioners formerly, and which no well-informed medical man at present would neglect. When any of the symptoms to be dealt with admit of a precise determination, such as the temperature, or heart's action, or others, it is no doubt highly advisable that instruments devised for the purpose should be employed to determine them; but after all these are only symptoms, and the condition of the system, and the powers of life we have to work upon, and to do what we can to prolong, embrace many things besides these, which equally claim our attention. There are forms of disease too in which the symptoms may be obscure, and neither the temperature nor the pulse afford satisfactory evidence of their existence, while the peculiar expression of the face, the attitude of the patient, or the particular mode in which he moves, though he be quite unaware of them himself, convey very important information to the observant practitioner, and enable him to arrive at a pretty correct conclusion, while others who have not observed them remain in doubt. This familiarity with the aspects of disease is of use to medical officers in another way; where duties are severe, men, from time to time, are anxious to evade them, and for this purpose will feign disease, but as they are seldom acquainted with the particular indications to which allusion has just been made, they omit them entirely, or, if a man have learned some of them, as occasionally happens, he is almost certain to overdo them, and in either case the observant officer can see through the attempt at deception.

While I remained on the active list, and had frequent opportunities of seeing them, it always struck me there was great room for improvement in the mode of recording the cases of men in hospital. The description of the case was in most instances diffuse, and but too often points of importance, which if present should have been noted, or, if absent, this should have been distinctly mentioned, were altogether overlooked. A well-kept case should include all such points, together with the changes of treatment from day to day, with the diet and extras; and should it terminate fatally, there should be, if possible, a full and comprehensive report of the appearances at the *post-mortem* examination, clearly and systematically arranged. To accomplish all this, of course, requires both method and practice, but any one who bestows these to an adequate degree upon them will soon find that he not only does his work better, but in less time than one who has given them no consideration. As a preliminary exercise in this direction I would recommend the younger medical officers to read over carefully any of their cases or reports, and strike out any word not necessary to convey their meaning, and I venture to say most will be surprised at



the number that can be omitted, not only without detriment, but actually with advantage to the force and clearness of their language.

It is the custom of the service for the medical officer in charge of any station or body of persons to draw up a report annually, in which to give an account of the various matters concerning the maintenance of the health of those under his care during the period, and the result of his practice. The preparation of this document is, I fear, occasionally regarded as of so little importance that it is thought anything will do that satisfies the official requirements in letter, however much it may fall short of meeting their spirit. This is not the temper in which the work should be undertaken; the perusal of such a report leaves on the mind of the reader an impression far from favourable of the writer's zeal and discrimination, and for his own sake he should avoid laying himself open to it. Medical officers, in writing these annual or other reports, should analyse the facts they have to do with most carefully, and in giving the results should employ the actual numbers upon which their conclusions are based, whenever that can be done, instead of general terms, which too often lead to habits of inexactness in the writer and convey very erroneous impressions to others. In the medical part of the report it is advisable to include short abstracts of important or fatal cases, with notes of the *post-mortem* appearances in the latter. A more detailed report of interesting cases should be made and retained for the sake of reference. I would strongly recommend the young officer to approach the subject with the desire to carry the analysis of the facts before him as far as they will permit, and after completing it to satisfy himself that his conclusions were legitimate inferences from them. If he will follow this plan I would further advise him to retain copies of his reports: he may wish in future years to utilise his previous experience, and, however vivid his impression of particular occurrences may be, he will often find that the perusal of what he wrote when the facts were fresh in his mind will supply some important detail regarding them that in the interval he had forgotten.

Facts carefully observed and marked are always valuable, and may prove so at a time and in a way never anticipated by the person who noted them. During the sittings of the late Committee of the House of Commons on the Contagious Diseases Acts, their opponents objected to the infecting and non-infecting sores being included, for statistical purposes, in the Army Medical Returns, under the term "primary venereal sores," and several witnesses on that side declared the classification unnecessary, as the infecting sores could in nearly every instance be distinguished from the non-infecting at various periods within a month from their first appearance. Witnesses in favour of the Acts supported the view that sores that never became

indurated might be followed by constitutional syphilis, and that this liability could not be indicated with certainty in any particular case until the secondary disease showed itself; but no actual numbers were given in support of the statement, nor its extent estimated, and the members of the Committee were evidently hesitating as to which side the weight of the evidence inclined. It had so happened that in 1842-3 I was in charge of the depôt of the 47th regiment in the West of Ireland, and that in the twelve months ending March of the latter year, out of a mean force of 254 men there had been 126 admissions for primary venereal sores, and in the next nine months 23 more occurred, of which 27 only were indurated, but 49 cases of secondary syphilis followed, showing that in this instance, even if every indurated case be supposed to have been followed by the constitutional disease, there are still about 45 per cent. of the secondary cases due to sores without induration. These facts, which came under my notice, and were recorded in my annual report nearly forty years previously, on being put before the Committee, virtually settled the question.

So much for the manner in which medical officers should utilise their opportunities for acquiring information, so as to enable them to take part advantageously in the discussion of a debating society. I will now pass to some other points which have an important bearing on questions connected with the causation and spread of some kinds of disease, which are not generally recognised.

At present there are no reagents for the causes of certain diseases, as yellow fever, dengue, cholera, except men or animals that are liable to them: consequently we cannot tell whether these causes be in operation in any locality, at any given time, unless men or animals who have been some time in the place contract the disease there at that time. It follows as a necessary result that the occurrence of such attacks merely proves that the cause was in operation in the locality when they occurred; but by themselves they can establish nothing as to its nature, whether it be a virus transmitted from man to man, or a miasm generated on the spot. These can only be arrived at from a consideration of the collateral circumstances. Take yellow fever, for example, at Port Royal, in Jamaica, or at the military hospital, Barbadoes. Men labouring under the disease may be introduced at either of these places, and persons in communication with them in the hospital may contract the fever, and were this a constant occurrence under such circumstances, or even a frequent one, it would be strongly in favour of the belief that yellow fever is transmitted from man to man if there were no evidence to the contrary; but there is a very great body of evidence to the contrary, and of the most decisive nature, from both places as well as elsewhere. I myself have seen large numbers of yellow fever cases introduced from ships at both places, during the



healthy season on shore, without any one brought into communication with them suffering, and I have seen at both stations cases of yellow fever spring up at another season among those on the spot, who had not been near any yellow fever case, and without any importation to account for it,—showing clearly that the cause of the disease may become active in these localities quite independently of previous cases brought from elsewhere. The evidence, when fairly analysed, therefore comes to this: First, that the cause of yellow fever may become active in a locality without the previous introduction of a case from elsewhere; Second, that where these causes are in abeyance in the locality, yellow fever cases may be introduced even in large numbers from elsewhere, and mixed up with other sick without any of the latter contracting yellow fever. Third, that when, after the introduction of yellow fever cases, persons who have communication with them contract the disease, it cannot be established that this is owing to a virus transmitted from the introduced sick, until the possible operation of local causes has been excluded, which, from the nature of the evidence, it cannot be. This illustration, in the present state of our knowledge, is no less applicable to dengue and cholera, and it may be to several other diseases.

Most of you, no doubt, are aware that those who believe that cholera is spread by man, maintain that choleraic diarrhoea is but the undeveloped form of the epidemic disease, and that the introduction of a case of choleraic diarrhoea is sufficient to originate an epidemic of true cholera. It is very generally admitted too, by those who hold this theory, that there are cases of cholera met with sporadically, presenting every symptom of the malignant form of the disease, and which, if met with during an epidemic, would be accepted without hesitation as typical instances of it, but which they object to admit as true cholera because they can neither affiliate them to a previous case, nor are they followed by an extension of the disease to others: that is, in plain language, when tested by their theory such cases do not fulfil its requirements, and therefore they cannot be true cholera. This mode of reasoning is altogether erroneous and deceptive, and I cannot too strongly impress upon you that, whenever you find facts at variance with your theories, it is high time for you to consider how far you must alter your theories to reconcile them with the facts.

The next point to which I will direct your attention is what has been variously denominated epidemic influence, epidemic cause, pandemic cause. In the present state of our knowledge these expressions merely indicate that factor, or series of factors, which lead to the recurring outbreaks of disease that are called epidemics, without attempting to specify their nature, though that be a legitimate subject for future inquiry. Some writers describe epidemic

influence as merely an idle fancy; others admit its existence, but would confine its operations within certain limits, which appear to them to result from their conception of its character; but with this, as with every other force in nature, after having recognised its existence, we must not attempt to tie it down to a certain line of operations we think it ought to follow, but we must find out how it acts from a careful consideration of all its manifestations.

To show the existence and power of epidemic influence, let us take small-pox, a disease, as we all know, readily communicated from those labouring under it to others who have not been protected by a former attack, or by vaccination, and which many believe depends on such communication alone for its prevalence. The number of deaths from this disease in London, from 1629 onwards, with a few blanks, are extant, and they show a continued succession of epidemic increases, recurring mostly at intervals of from two to four years. From 1701 to 1800 inclusive, there were 38 of such epidemics well marked. From 1801 to 1831 the disease caused much less mortality, partly in consequence of the introduction of vaccination, and limitation of inoculation; but it still continued to fluctuate as previously. From 1841, when there was an epidemic, to 1881 inclusive, there were eleven distinct epidemics, or, deducting the first, ten in forty years, or one about every fourth year. In Paris, Vienna, Berlin, and other places, this disease pursues a similar course, epidemics recurring about every fourth year, and in Paris these seem to culminate the year preceding that in which they reach their highest point in London; while in Scotland and Ireland, since the registration of deaths commenced, those from small-pox have attained their maximum the year after London,—facts which indicate that at this point of Europe the epidemic influence, so far as small-pox is concerned, moves to the north and west.

The registration of deaths in India has been gradually accumulating a most valuable body of evidence with reference to disease in that country, from which it appears that there was a severe epidemic of small-pox in the Madras Presidency in 1868, which declined there in 1869, but that year Berar, Oudh, the North-West Provinces, and Punjab, all suffered very severely, the disease declining in them in 1870. In 1871 small-pox was again active in the Madras and Bombay Presidencies; this culminated in the latter and in Berar in 1872, and in Oudh and the North-West Provinces and Punjab in 1873. The death-rate from small-pox was considerably higher in the Madras Presidency in 1873 than in 1872, while in Bombay it had fallen very much, and in 1873 the Madras rate was but slightly reduced, while those in Bombay and Berar were very low indeed; the disease remained severe in the Central Provinces, however, though everywhere else to the northward it had decreased largely.



In 1877 there was another severe epidemic in the Madras and Bombay Presidencies and Berar, which in 1878 extended to the Central Provinces, North-Western Provinces, and Oudh and Punjab, culminating in the North-Western Provinces that year, but in 1879 in the Central Provinces and Punjab. In 1880, as in 1870, small-pox was again low all over Hindostan. There were thus in the ten years, 1868 to 1877, four distinct epidemics of small-pox in the Madras Presidency, three of which passed northwards to the Punjab, the fourth not extending beyond the Central Provinces. It has been noticed above that the epidemics in Western Europe pursue a northerly and westerly course, and it is particularly worthy of notice here that the same outbreak in Madras in 1868, which reached the Punjab in 1869, was followed, in the beginning of 1870, by an epidemic in the South of France, which gradually extended to Paris, and embraced London in 1871, and Scotland and Ireland in 1872. The epidemic which was in force in Madras in 1877, and in the Punjab in 1878 and 1879, was followed, in the latter part of 1879, by increase of small-pox at Marseilles, and an epidemic in Paris in 1880, and this, in its turn, led up to that of 1881 in London. We have no information as to the prevalence of small-pox in the countries between India and the South of France, in 1869 and 1879, to complete the connection between the two sets of epidemics, but the same direction of movement, and sequence of dates, indicate the operation of a force sufficient to determine the progressive epidemic frequency of small-pox over an immense area of the earth's surface, according to determinate laws,—in short, the epidemic influence, or cause.

Though the epidemic influence invade such extensive areas the disease which arises under it is never equally diffused over them; it springs up at various points, frequently distant from each other, and is rapidly developed; at other points variously distributed between these it soon appears in less force, and mixed up with the latter there are considerable tracts with very little. As the disease declines in the places first mentioned it not uncommonly becomes aggravated in some of those where it had been going on with less activity, while in the remainder of this category it may gradually disappear without any such exacerbation. Any one desirous of studying this matter for himself may find the materials for the small-pox epidemic of 1871–2 in England and Wales, at page 184 of the Registrar-General's Annual Reports for those years, or in more detail in the Quarterly Reports for the same years. Similar details for other diseases that became epidemic are to be found in the volumes for the years in which they occurred, and they display the same general features as here described for small-pox.

I will now offer some remarks on another disease, which most of those present are acquainted with, and which is of much interest



in itself; I allude to malignant cholera, upon which so much has been written, and concerning which there is still so great diversity of opinion, while surprisingly little has been accomplished in arriving at the real facts, and in placing what is known concerning them on a satisfactory basis. It appears to me that much may be done to simplify this question, and as many now present may be brought into contact with cholera in the course of service, this seems a suitable occasion for bringing the subject under their notice, as they may have opportunities for investigating points which are in want of elucidation.

The late Dr. Bryden, who stood highest among writers on cholera, as well as for the acuteness and discrimination he displayed in arranging and classifying the mass of material at his disposal, as in the extended grasp he had of the whole subject, showed there was an extensive district in Lower Bengal, which he called the endemic area, where cholera had been present for many years, and where, from time to time, it became epidemic. On such occasions it frequently passed to the north-westward, out of the endemic area, to countries where it was less commonly met with, and then usually as a well defined epidemic, and which, for this reason, he named the epidemic area. When a cholera wave proceeds from the endemic area it usually causes the disease to appear in an epidemic form, covering the country more or less completely up to about the  $80^{\circ}$  of east longitude in the first year. The epidemic culminates at different points, in various months, according to local circumstances, but usually in August or September, after which the frequency of the attacks is much reduced; it commonly reappears the following spring, but with less intensity, unless reinforced by a fresh wave. The epidemic may not proceed beyond the  $80^{\circ}$  of longitude, in which case the portion of the North-West Province to the west of this and the Punjab may escape; when, however, during the first year, the high ground of Chota Nagpoor and the districts of Banda and Hamirpur, south of the Jumna, come under it, an epidemic, during the following one, over the country beyond  $80^{\circ}$  east, is imminent. This likelihood is strengthened when sporadic cases occur at distant points, from Delhi northwards, in December and January, circumstances which have frequently preceded epidemics there, and which indicate the *aura* of the epidemic, as Dr. Bryden called it, and show that the factors which give rise to cholera were already active so far in advance, though the season was not suitable for their extensive development. When a cholera epidemic does spring up in the country beyond the 80th meridian, it usually includes Lahore, but does not extend westward beyond the 74th until a subsequent year.

Bryden, Cornish, and others were of opinion that the cholera epidemics of the West and South of India, as well as those of the

North, proceeded from the endemic area in Lower Bengal, though they differed as to the mode by which they were disseminated. The occurrences of 1874 and 1875, however, raise a doubt on this question; in the former year cholera was epidemic to the eastward of a line from Gangam through Benares to the foot of the hills; everywhere else there was a complete absence of the disease as an epidemic, and but a few sporadic cases. In Bengal, beyond the area occupied by the epidemic, there were 68 deaths registered from cholera in Oudh, 29 in the districts of the North-West Provinces south of or bordering on the Jumna, 462 in those to the northward west of  $80^{\circ}$  east, and 73 in the Punjab up to  $74^{\circ}$  east. In the Madras Presidency there were 42 deaths from cholera in 1874 in the Tanjore and South Arcot districts, and 13 in Malabar. In the Bombay Presidency there were 19 deaths from cholera in Bombay city, and 10 in the Ahmedabad district. In 1875 cholera became active in Ceylon in January, in April in the Tanjore district in Madras, and at several points in Bombay, and by July had covered the whole of Hindostan, with a few intervals, from Cape Commorin to Lahore as a severe epidemic. The cases in the North-West Provinces in 1874 may be set down, in ~~fact~~, to the remnant of a slight epidemic which had passed over them in 1873, and in the latter months to the *aura* of another which had been strongly developed in the districts of Gorakpur and Basti, on the borders of Oudh, in September 1874, and which spread over all these in 1875 as already stated; but those in Ceylon and Madras are altogether different, and it is difficult to reconcile them with the theory of either Bryden or Cornish. The distribution of cholera in 1880 bore a very close resemblance to that in 1874, in the absence of epidemic activity in Ceylon and the South and West of Hindostan, and it will be very interesting to watch, on the next diffusion of the disease, whether these tracts be invaded in a similar or different manner from what took place in 1875.

In Europe, cholera epidemics, though less regular than in India, still manifest the same general character, and display more or less most of the features they present in the latter. They usually commence at some point in the south, and advance northward; the following year they continue in less force in the area thus covered, and make a farther advance, after which they disappear. On other occasions during the first year the *aura* of the coming epidemic, to use the expression of Dr. Bryden, precedes the main body so far as to lead to single cases, or small groups of malignant cholera, usually accompanied by considerably increased numbers of ordinary summer cholera in this country, as at Southampton, and at Theydon Bois, in Essex, also at Copenhagen, and in the southern districts of Sweden, in 1865, which was followed up by the disease in the epidemic form in 1866. The outbreak at Altenburg, in Germany, in



1865, which remained isolated that year, but was followed by the epidemic in 1866, is to be explained in the same way. Sometimes the disease does not advance as an epidemic beyond the area it occupied in its first year there, but the following year the countries to the northward have an unusual number of sporadic cases of malignant cholera, together with much simple cholera and diarrhoea with cramps and vomiting; the cholera epidemic in the South of Europe in 1867 was followed by such an outbreak in this country in 1868, and a similar one occurred at Copenhagen that year. In this camp in 1868 the troops under canvas had a good deal of this diarrhoea and some ordinary summer cholera at the end of July and beginning of August, and a soldier of 2/6th regiment was attacked with well characterised malignant cholera on 12th August, and died of it on the 13th. Another set of conditions under which ordinary summer cholera, interspersed with sporadic cases of the malignant form, became unusually numerous—and even three considerable groups of the latter description of cases occurred—took place in 1859. At this time cholera was prevalent in the west of the Mediterranean, and in summer was epidemic in Belgium, Holland, Hamburg, and on to Sweden. The deaths from cholera in London, which in 1858 were 49 per million living, rose to 71 in 1859, and fell to 18 per million in 1860; and in 1859 three groups of the malignant form of the disease presented themselves; at the Coast Guard station at Woolston, near Southampton; at Glass Houghton, in the parish of Castleford, near Pontefract; and at Wick, in the North of Scotland. You will notice particularly in all these cases (and more could have been given) the increase of the ordinary summer cholera, the greater frequency of the sporadic cases of the malignant form, and the occurrence of small outbreaks of the latter all taking place together, on the approach of cholera as an epidemic.

From these facts you will perceive that cholera, as an epidemic, is not a thing of one station, or even one country, but involves large geographical areas in the same year, and successive areas in consecutive years. Its essential factors must therefore operate over spaces of equal extent, with the power of occupying fresh ground year after year as we see it do, constituting epidemic influence, in short, as has already been shown to affect small-pox. It must not be concluded, however, that the epidemic influence in small-pox and in cholera are the same; not only may the epidemic element in each consist of more factors than one, but in the case of small-pox it has been clearly established that the *materies morbi* is largely augmented in quantity in the system of each person who is attacked, and this can be communicated to healthy persons readily, so as to excite the disease in them. As regards cholera, however, this certainty of an increase of the cause of the disease in the system of those labouring under it is wanting, and the belief that cholera evacuations,

passed into water, convey the *materies morbi* and so cause the disease in those who drink it, is merely an inference from circumstances which do not uphold that conclusion.

When any effect is found to result from the combined operation of two causes, one of which is of very extended influence, while the other is very limited in this respect, if the contribution of the latter to the common result cannot be established by direct experiment, it can only be arrived at after an approximation at least to the influence of the former has been made, so as to enable it to be excluded. If this be not done much that is due to the general cause is set down to the limited one, and great error committed. In the case of small-pox, a small particle of matter inserted below the cuticle, in a susceptible subject, leads to an eruption, it may be, of hundreds of pustules, the matter in any one of which will serve to produce the same disease in a dozen of others. In cholera there is nothing analogous to this: the discharges of an individual labouring under this disease may get mixed with drinking water, and persons who have used this water may get cholera, but the fact of there having been a previous case indicates that the epidemic factors were in operation at the place at the time, and it is only by ignoring their operation, and setting down the effect due to that to the contaminated water alone, that the inference has been drawn that the cholera stools contained the *materies morbi*, and that that was multiplied in the body of the sick.

It is a recognised fact in India, and the more limited experience of this country has afforded many examples of the same, that while the cholera influence is present, the employment of saline purgatives, or even the milder vegetable or mixed laxatives, such as Gregory's powder, is very apt to be followed by an attack of the disease; the use of tainted meat or fish, or ~~a~~cescent fruit, or a debauch, or even a hearty supper, have all of them been frequently followed by attacks of the disease, without any ground for supposing they had in any way been contaminated with cholera evacuations. With such facts well ascertained, we are not justified in assuming that the power of water containing cholera evacuations is due to the presence of the *materies morbi*, or to anything more than decomposing organic matter, as in the case of meat, fish, or fruit, undergoing a similar change. The recognition of this view, as you will perceive at once, must lead to a profound alteration of the opinions held in this country regarding the causation of cholera and the measures recommended for its prevention.

It may be objected to this view, and no doubt will be, that when a pure water supply has been substituted for a contaminated one cholera has been much reduced. Now assuming there has been no alteration in the incidence of the epidemic influence, this is precisely the result to have been anticipated from the substitution.



Water is so largely used by everybody, that the reduction of any contaminating matter in it, capable of exciting the disease when the epidemic influence is present, must be accompanied by a diminution in the number of attacks, and it would be the same whether it be assumed that the active ingredient was the *materies morbi*, or only decomposing organic matter, or even sulphate of magnesia. The argument then does not sanction the inference drawn from it. As an illustration of the erroneous conclusions which may be arrived at when the variation in the incidence of the disease is overlooked, it was maintained recently that, owing to the introduction of Loch Katerine water, the mortality from cholera in Glasgow, in 1866, was much less than in previous epidemics, when the supplies were obtained from objectionable sources. The facts that the epidemic of 1866 in Scotland was confined in a great measure to the east coast, and that scattered cases only presented themselves in the western districts of the country, without assuming the proportions of an epidemic anywhere, and that the mining and manufacturing towns and villages round Glasgow, which suffered severely in previous epidemics, and which still depended on their former sources of water supply, and had not the advantage of that from Loch Katerine, presented a similar immunity from cholera as Glasgow itself, were altogether overlooked. It is but necessary to mention these facts to show how erroneous the inference was.

The low mortality from cholera in England in 1866, as compared with 1849, led many to the conclusion that the amelioration was due to the introduction of preventive and sanitary measures suggested by the experience of 1849 and 1854. No doubt these measures were several of them beneficial where they were applied, but the reduction of mortality was not confined to such places, but was met with over the country at large, including villages, villages growing into towns, but still with village organisation, and small towns with little or no improvements in their sanitary arrangements. These statements are borne out by the following figures:—In 1849, throughout England and Wales the mortality from cholera was 30 per ten thousand living; in 1854 it was 11 per ten thousand, and in 1866 it amounted to 7 per ten thousand only. Accompanying this reduction of mortality, of the 623 registration districts in England and Wales there were only 14 per cent. of them in which no death from cholera was recorded in 1849; in 1854, with a reduction of mortality from 30 to 11 per ten thousand, the number of districts without a death was 26 per cent.; and in 1866, with a further reduction of mortality to 7 per ten thousand, the districts without a death from cholera had risen to 38 per cent. These facts show a reduction in the prevalence of cholera too great and far too general to have been effected by sanitary measures adopted to a limited extent, and can only be explained by a diminished inci-

dence of the epidemic influence over the country for the time being, but which may disappear on the advent of another epidemic. The experience of such an epidemic will open our eyes to the true value of a good many measures which were deemed to have prevented an increase of cholera in 1866—an increase, as the statistics just quoted show, which never really threatened us.

Those who think that cholera is transmitted from man to man hold that the disease never travelled faster than man, and wished it to be inferred that man himself was the active agent in diffusing it. The Congress at Constantinople, in 1866, affirmed both propositions, and expressed their belief that as the rapidity of travelling had been much accelerated, both by land and by sea, it would be found that, in future, epidemics of cholera would spread from India more rapidly, and to greater distances than previously. The Congress at Vienna, in 1874, while adopting the view that cholera did not travel faster than man, considered prophecy was no part of their functions, and therefore abstained from stating how they thought acceleration of locomotion would influence its diffusion. There was sufficient evidence in 1866 to have led the Constantinople Congress to hesitate in formulating such sweeping statements as it did on this question, and the experience of the next eight years induced greater caution in that at Vienna, which, though it adopted the leading point, still showed a disinclination to reassert its obvious development. In India, where a man can now go from Calcutta to Lahore in fewer days than it took months to accomplish forty years ago, cholera epidemics travel no faster than they did then, and any one who will study the maps in the Annual Reports of the Sanitary Commission with the Government of India for the last ten years, will find that railways ran into, or out of, or along side cholera fields at different points, without appearing to influence the extension of these fields at any of them. In the Appendix to the Army Medical Report for 1880 there is a notice of a paper by Professor Pettenkofer, read before the Medical Society of Munich, in which reference is made to the Report of the Cholera Commission for the German Empire, which has been recently completed, and Pettenkofer is of opinion, from the general review of the spread of the epidemic in the whole of Germany, as given by Hirsch, that any one looking at the charts and observing the sharply-defined limits of localised epidemics must come to the conclusion that mere intercourse with cholera cases, or cholera-stricken localities, had nothing to do with its spread, but that the most important part was played by the locality itself to which the disease germ was brought (p. 252). Dr. Pistor, who described the spread of cholera in the Province of Oppeln, while he thinks the germ of cholera was always introduced, states: "It has a certain relation to human intercourse, but it does not cling to the lines of



communication in such a way, for instance, as that places along the lines of railway are exceptionally attacked" (p. 255). The experience of this country has been to the same effect. Though great additions were made to the railway mileage between 1849 and 1866, and the numbers of passengers increased enormously, it has been shown above the successive epidemics diminished in severity, and the areas of the country which escaped the disease increased.

Drs. Pettenkofer and Pistor, having no idea of an epidemic influence in cholera, have to fall back on communication by man to account for the introduction of the germ from which they derive these epidemics. The experience of Dr. Bryden in India enabled him to point out the influence of the *aura* of an advancing epidemic to excite undoubted cases of malignant cholera far beyond the limit where the disease had been developed as an epidemic, and the instances cited above in 1859 and 1865 show the same thing in Europe; the epidemic influence then must supply the factor necessary for this purpose, and the agency of man is not required. It would be interesting to learn how this factor is diffused, but at present, though there are a number of facts bearing on it in different ways, they scarcely do more than indicate the probable line of inquiry that must be pursued to clear it up; but, inasmuch as many of you may be placed under circumstances which may enable you to prosecute the investigation, it is well to mention them.

At several of the stations occupied by the American troops in the Rocky Mountains, fever of a remittent form appears from May to July, which yields readily to quinine; there is no known source of malaria near those stations, and the season is not that at which malarious emanations arise in other parts of the States. It was observed, however, that the river water proceeding from the melting snow contained a large amount of organic matter, and that the first heavy fall of snow also contained a large quantity, but in subsequent falls it decreased. As the melting began to fill the rivers in spring the fever appeared among those using the water, and Dr. Smart, the officer who recorded the facts, considered the organic taint to consist of vegetable emanations and *débris*, swept up by the winds from the face of the continent and precipitated by cold and moisture along with snow from the higher regions of the atmosphere (Army Medical Department Report, 1877, p. 192). If malaria can be transported in this fashion, which there is good ground to believe it was in the instance just quoted, there seems no reason why the exciting cause of cholera, supposing it to be particulate, might not be carried a long way in the atmosphere, and be brought to the ground either by a shower of rain, or a descensional current of air, arising from other causes. In connection with this the outbreaks of cholera met with in India after unusual or heavy rains, or after

dust-storms or other atmospheric commotions, are specially worthy of notice.

The only direct observation there is, so far as I am aware, of what was supposed to be the cholera poison was made early in 1832, by Dr. Prout, when engaged in ascertaining the weight of atmospheric air. After many determinations, extending over several weeks, the wind came round to the east, and he found an immediate though small increase in the weight. Thinking some error had been committed he went over the process again, but still obtained the same result, and the increased weight remained while his experiments continued, which they did for another six weeks. A day or two after this shift of wind and increase of weight of the air were observed the first case of cholera was reported in London. Dr. Prout attributed the increase of weight to some kind of malaria heavier than air, which had become mixed with it, and which was in some way connected with the appearance of cholera.

In his first report on the epidemic cholera Jameson referred the motion of the disease to the north-westward to the action of the wind, during the south-west monsoon, carrying the miasm along with it; and Dr. Bryden has since advocated the same view, and greatly enlarged and systematised the information bearing upon it. At present his theory accounts satisfactorily for much that is known regarding the progress and distribution of cholera epidemics; but until the winds, with the changes they undergo during the year, and the currents in the atmosphere above them, have been fully studied, we shall not be in a position to accept it entirely. In the meantime, a peculiarity in the distribution of cholera, pointed out by Dr. Bryden years ago, viz., that epidemics leaving the endemic area were limited to the north-west by a line about the  $80^{\circ}$  east longitude in the first year of their progress, has recently received a satisfactory explanation, it having been shown that during the south-west monsoon the winds from the Bay of Bengal meet those from the Arabian Gulf in that neighbourhood, and so check the onward progress of both in their original directions. Along the line of this meeting there must be ascensional currents, which pass away at a high elevation, carrying with them any cholera miasm they may contain, and distributing it at any point at which they may reach the earth again.

An occurrence which took place so long ago as 1848, which has never been fairly understood, has a very important bearing on this question. A ship, the "Swanton," with German emigrants on board, left Havre on 31st October, for New Orleans; on 25th November there was a very hot south-east wind, such as the captain had never felt before, and which he described as "more like artificially heated air than anything else." On 26th November, after this wind had lasted for about twenty-four hours, the first case of cholera



appeared; the ship was then in lat.  $25^{\circ} 47' N.$ , and long.  $57^{\circ} 8' W.$  The "Swanton" arrived at New Orleans on 11th December, having lost thirteen passengers by cholera. Another ship, the "New York," left Havre on 9th November, also with German emigrants and others on board, for New York. On the 24th November there was a very chilly cold wind, and "there was a general overhauling of baggage for warm clothing." The next day, the 25th, became exceedingly hot, with a south-easterly wind, and the first case of cholera occurred, the ship being in lat.  $42^{\circ} N.$ , and long.  $61^{\circ} W.$  This ship reached New York on 1st December, having lost seven passengers, and landed eleven others with cholera. Cholera was prevalent in Germany at the time these vessels left Havre, but it had not reached the West of France. It is stated, however, that there was an emigrant on board the "New York" "who had clothing that had belonged to an individual who had died in Germany of cholera. During the day of intense cold some articles of this clothing were taken from the chest, and were worn by several of the passengers, and these passengers were the first taken on the ship" ("Cholera Epidemic of 1873 in the United States," p. 608). These German emigrants had been at Havre about two months before they sailed; a doubt was expressed at the time, however, whether some had not come from Germany shortly before sailing, but whether these were in one of the ships only or were distributed between both does not appear.

The question at the time arising out of these occurrences was, how did the passengers in these ships contract cholera in the middle of the Atlantic? One set of epidemiologists said the cause of the disease had been conveyed from Germany in clothing, and was communicated to those who wore it, after being unpacked, on 24th November. Though the narrative be wanting in many particulars, as to localities and dates, we should expect to be furnished with now, before coming to a conclusion on the subject, it might have been accepted had the "New York" only been concerned; but it was quite inapplicable to the "Swanton," which had not cold weather on the 24th, nor any overhauling of chests for warm clothing. Another set of epidemiologists, seeing that a change of wind to the south-east took place with both ships before cholera appeared, were of opinion something must have been conveyed by that wind to cause the disease in both, but where it came from they were unable to indicate. The remark of the captain of the "Swanton," that it was "more like artificially heated air than anything else," affords a clue to the mystery, that now enables it to be explained. Any one who has experienced a hot wind, with which no doubt most of you are familiar, will see at once this is what he described; but as a hot wind always originates over arid land, how could such a wind have been experienced from the south-east at the place where the

“Swanton” was, from which a south-easterly line would pass through the South Atlantic clear of all land? The wind was really from the desert in North Africa. In November, when the north-east trade is being re-established over the Northern Atlantic, it reaches to  $6^{\circ}$  N.; and about longitude  $30^{\circ}$  W., and latitude  $10^{\circ}$  to  $20^{\circ}$  N., at this season, north-easterly winds are frequently experienced of the character described by the captain of the “Swanton,” and, in addition, often bearing red dust in such quantity as to cover the sails and rigging of passing vessels, leaving no doubt as to their place of origin, and further illustrating their transporting powers. Such a current of air which did not come to the surface of the sea, but continued to flow at some elevation above it, would retain its peculiarities for a long time: it would pass on to the inner limit of the trade, about  $6^{\circ}$  N., and there rising in the atmosphere as the air of the trade does, will then double back to the north-west, constituting a south-east wind, and would reach the “Swanton” as such as described above. In November, 1848, when these occurrences took place, there had been cholera in Egypt and along the North Coast of Africa, and with a moderate velocity of twenty miles an hour the wind might have transported emanations from that to the “Swanton,” in from ten to twelve days. It will not do to speak too dogmatically on the subject from this instance alone; but this illustration, with others I have brought to your notice, will show its extent, and will indicate the spirit in which it should be investigated.